

# EXPOSING THE REVOLVING DOOR IN EXECUTIVE BRANCH AGENCIES

Logan P. Emery

Rotterdam School of Management, Erasmus University

and

Mara Faccio\*

Purdue University, NBER, ABER & ECGI

**Abstract:** We develop the first comprehensive mapping of the revolving door phenomenon in the U.S. by examining the work experience in executive branch agencies of 1,910,150 individuals covering top corporate positions in 373,011 firms. The phenomenon is highly prevalent, present in one out of every three public firms. Consistent with the “knowledge view” of the revolving door, we show that direct transitions to the private sector (i.e., within three years of leaving an agency) tend to occur in response to *increases* in regulatory activity or the incidence of fines. Those transitions tend to be *followed* by benefits in the form of an increased incidence of procurement contracts. Possibly consistent with the “quid pro quo” view of the revolving door, we document that transitions that immediately follow presidential elections tend to be *preceded* by an abnormally low incidence of fines and an abnormally high incidence of procurement contracts.

JEL classifications: D72; K23; G38; L51; J45

Keywords: revolving door, government, enforcement actions, regulation

\* We are grateful to Laura Field, Elisabeth Kempf (discussant), John Matsusaka, Ben McCartney, Deniz Yavuz, and seminar participants at Ball State University, the City University of Hong Kong, La Trobe University, the London School of Economics, the National University of Singapore, the University of Delaware, the University of Groningen, Xiamen University, the International Virtual Research Seminar in Finance co-hosted by Aalborg University Business School, the BAFFI CAREFIN Centre (Bocconi University), the Centre for Governance, Regulation and Industrial Strategy (University of Bath), the Grenoble Ecole de Management and Università Sapienza (Rome), the 2021 American Finance Association annual meeting, and the 2021 Finance Conference in Honor of Phelim Boyle (jointly hosted by Wilfrid Laurier University, the University of Waterloo, and the Fields Institute) for valuable comments and suggestions. The authors gratefully acknowledge financial support from the “Blake Family Fund for Ethics, Governance and Leadership.”

The flow of personnel from government to the private sector (“the revolving door”) has been the subject of numerous academic studies and regulatory debates. Yet, no comprehensive mapping of its prevalence exists, and the evidence that does exist is either anecdotal<sup>1</sup> or confined to specific industries or settings.<sup>2</sup> This lack of clarity is somewhat unsettling, as the revolving door presents a pressing ethical concern. Critics of the revolving door phenomenon observe that regulators in pursuit of a job in an industry may signal their interest to the industry by acting leniently. Advocates of the revolving door instead observe that the possibility of a transition to the private sector generates incentives for regulators to invest in their human capital to develop valuable knowledge and expertise while in the public sector.

With this in mind, we develop the first systematic mapping of the revolving door phenomenon in the U.S. across firms, industries, regions, and over time. We then use event study techniques to study the dynamics of the revolving door and investigate the motivations behind it.

Specifically, we examine the prior work experience in executive branch agencies of 1,910,150 individuals with career histories available in *BoardEx*.<sup>3</sup> These individuals cover top corporate positions in 373,011 unique firms during 2002-2018. We document that one out of every

---

<sup>1</sup> The names of Dick Cheney, Dan Coats, Linda Fisher, Dick Gephardt, Philip Perry, Donald Rumsfeld, and Pat Toomey may come to mind.

<sup>2</sup> Bien and Prasad (2016) document that 15 of the 55 medical reviewers who reviewed oncology drug approvals at the Food and Drug Administration (FDA) between 2001 and 2010 subsequently either obtained jobs at biopharmaceutical firms or acted as consultants for the biopharmaceutical industry. For a sample of 994 publicly traded financial firms, Shive and Forster (2017) document that 31% of the firms have at least one board member or upper-level executive with prior experience at the Federal Reserve, the Office of the Comptroller of the Currency (OCC), the Federal Deposit Insurance Corporation (FDIC), the Securities and Exchange Commission (SEC), the Commodity Futures Trading Commission (CFTC), or the Financial Industry Regulatory Authority (FIRA). Tabakovic and Wollmann (2018) show that nearly 30% of patent applications are submitted by firms that have hired at least one former United States Patent and Trademark Office (USPTO) patent examiner. Tenekedjieva (2020) reports that, during 2000-2018, 38% of insurance commissioners move to the insurance industry after their term expires.

<sup>3</sup> A different set of papers investigate the opposite path, i.e., transitions from an industry to its regulatory agencies. An example is Gormley (1979), who studies flows of personnel from the broadcasting industry to the Federal Communications Commission (FCC), and documents that such transitions are associated with an increase in the likelihood of decisions that are favorable to the broadcasting industry. Cohen (1986) extends Gormley’s analysis dynamically by examining both the entrance and the exit patterns of FCC commissioners. In contrast to Gormley (1979), he finds industry background of FCC commissioners to be unimportant in explaining their voting behavior, while he finds that commissioners who are subsequently hired by the industry are those who were less supportive.

15 firms in the sample has at least one individual in a top corporate position who has prior work experience in one of 258 U.S. executive branch agencies from the *Federal Register* that appear in *BoardEx* (we refer to these individuals as “former regulators”). This fraction increases dramatically with firm size: one out of every three publicly traded firms has at least one former regulator among the individuals who cover top corporate positions. A substantial fraction of these individuals consists of former executive branch employees who transitioned to a top corporate position within three years since leaving an agency (following Cohen (1986), henceforth, we refer to these as “direct transitions”).

We document that direct transitions do not occur indiscriminately. Rather, these former regulators tend to be appointed from the agencies that are more relevant to the firm, and the timing of those transitions strongly correlates with actions by the relevant agencies. In particular, direct transitions tend to endogenously occur around increases in regulations for that apply to the firm.

The panel structure of the data combined with the dynamic event study framework we employ also allows us to investigate in detail the presence of two types of possible benefits that may accrue to firms in the years that surround a regulator’s transition to (or departure from) the firm. We examine leniency in enforcement, as proxied by a lower incidence of fines, and preferential treatment in the awarding of procurement contracts, as proxied by a higher incidence of procurement contracts.

Previous studies of the revolving door have focused on two non-mutually exclusive hypotheses on how firms benefit from the revolving door. The first hypothesis states that former regulators are appointed in exchange for favors provided to the firm *before* leaving the regulatory agency. We refer to this as the “quid pro quo” hypothesis. Examples of benefits include lenient monitoring, preferential treatment in the awarding of contracts, tighter restrictions over entry of

new rivals, etc. An example of quid pro quo is former Principal Deputy Undersecretary of the Air Force Darleen Druyun. Druyun pleaded guilty to a corruption felony and was sentenced to nine months in jail for inflating the leasing price of a fleet of 767s (to \$23.5 billion) in a contract described in the media as favorable to her future employer, Boeing.<sup>4</sup>

The second hypothesis states that former regulators are appointed for their knowledge and expertise. We refer to this as the “knowledge” hypothesis. While this hypothesis does not necessarily make any predictions about ex-post firm behavior, appointing former regulators could lead to benefits subsequent to their transition to the private sector, either because of the technical knowledge the former regulators possess or because of their personal connections.

Interestingly, regardless of how benefits are measured, we find no systematic evidence of benefits prior to or concomitant with direct transitions from government to top corporate positions. Consistent with the “knowledge” hypothesis and Che’s (1995) signaling model,<sup>5</sup> we find that direct transitions from government to top corporate positions tend to occur concomitant with an increase in aggressiveness by the enforcement agency in question. Specifically, we find that transitions from a fine-imposing agency tend to occur concomitant with an abnormally high incidence of fines issued by that agency.

We also find that firms tend to benefit *following* the appointment of former regulators. In the years that follow direct transitions, firms tend to be awarded procurement contracts with a

---

<sup>4</sup> Cashing In For Profit? Who Cost Taxpayers Billions In Biggest Pentagon Scandal In Years? CBS News “60 Minutes”, 2005, <https://www.cbsnews.com/news/cashing-in-for-profit/>

We stress that a purpose of this study is to investigate the extent to which the prospect of top corporate jobs in the private sector appear to be related to lenient regulatory behavior prior to the transition. This study, on the other hand, is not aimed at addressing the question of the prevalence of corruption in government agencies. Clearly, top corporate jobs could be a payoff, but do not need to. Less highly ranked jobs or jobs for family members may provide a valuable alternative. Also, cash payments or various perks could also be used as part of a quid pro quo scheme.

<sup>5</sup> In Che’s (1995) signaling model a regulator of unobservable quality signals her quality to the industry, and any perspective employers, through aggressive (rather than lenient) monitoring,

significantly higher frequency. The subsequent departure of former regulators is then followed, at least temporarily, by a drop in the number of procurement contracts for a given firm.<sup>6</sup>

A natural concern with the documented benefits is that they may accrue to the worst firms rather than to the best, potentially endangering allocative efficiency (Stigler, 1971, Peltzman, 1976, Schoenherr, 2019, Haselmann, Schoenherr, and Vig, 2021). If so, those contracts would likely be executed worse and/or renegotiated more often. Contrary to this conjecture, we find that procurement contracts allocated to firms with former regulators tend to be renegotiated *less* often than contracts that were allocated to the same firms prior to the appointment of former regulators. We also find no evidence of larger price increases for those firms with former regulators that do renegotiate. Thus, the benefits that accrue to firms *ex-post* do not appear to involve disproportionate increases in cost to the government. Rather, the opposite is true. Thus, it does not appear that the higher incidence of contracts reflects preferential treatment of “poor quality cronies” by government agencies.

It is interesting that the evidence documented for the average firm casts a positive view of the revolving door. The negative view of the phenomenon (“quid pro quo”) involves illegal (corrupt) behavior of government employees who, explicitly or implicitly, use their position in government to land a job in the private sector. It might well be the case that many, perhaps most, former regulators in our sample did not have sufficient power or incentives to engage in such illegal behavior. If so, our tests may lack the necessary power to detect evidence in support of the “quid pro quo” hypothesis. Absent a clear measure of both power and incentives, it is difficult to

---

<sup>6</sup> Indirect (i.e., later) transitions should be less likely to provide evidence supportive of either revolving door hypothesis (Gormley (1979) and Cohen (1986)). On the one hand, former regulators who joined a firm, say, fifteen years after leaving an agency could have not provided agency-related favorable treatment in the few years that immediately precede their transition to the firm. Furthermore, their knowledge (of either current technical knowledge, or connections knowledge) is also likely to be severely diminished. Therefore, we use indirect transitions as a counterfactual. Consistent with a weakened role of agency experience and knowledge, we find no evidence of benefits for indirect transitions involving former regulators who joined the firm more than three years after leaving government.

overcome this hurdle with the data available. We do, however, present some circumstantial evidence by exploiting “expected” exits from government positions. In those instances, the incentives to provide lenient treatment to the industry, in the hope of landing a job after the exit, are relatively higher. Presidential elections provide an intuitive setting to test this hypothesis. It is well known that, following presidential elections, high ranking Presidential appointees are often replaced by new ones chosen by the new President. The timing of elections is associated with an increased risk of termination. Thus, it is possible, and perhaps even likely, that agency employees who expect to be replaced following a Presidential election will act leniently immediately prior to the election to facilitate their transition to the private sector. Using this setting, consistent with the “quid pro quo” view of the revolving door, we find evidence of both an abnormally low incidence of fines and of an abnormally high incidence of procurement contracts prior to transitions that are induced by Presidential elections.

Thus, while the evidence overwhelmingly supports the “knowledge” view of the revolving door, some evidence of “quid pro quo” is found in a more narrow/limited setting.

Our main findings relate to the literature in the three following ways. First, contrary to Tabakovic and Wollmann (2018) and Tenekedjieva (2020), we find no pervasive evidence in support of the “quid pro quo” hypothesis. Tabakovic and Wollmann (2018) study transitions from the U.S. Patent and Trademark Office to the private sector and find evidence of leniency being on average rewarded. In particular, they document that patent examiners who grant more patents to a firm are more likely to be subsequently appointed by that firm.<sup>7</sup> Tenekedjieva (2020) studies

---

<sup>7</sup> In the private sector, Cornaggia, Cornaggia, and Xia (2016) document that analysts on average provide inflated credit ratings to the firms that subsequently hire them. Studies by Blanes i Vidal, Draca, and Fons-Rosen (2012) and Bertrand, Bombardini and Trebbi (2014) also find evidence consistent with the “quid pro quo” hypothesis in the context of revolving-door lobbyists. A larger literature on corporate political connections documents systematic evidence consistent with the ex-post “quid pro quo” hypothesis both internationally as well as in the U.S. (see, for example, Sapienza, 2004, Khwaja and Mian, 2005, Faccio, Masulis, and McConnell, 2005, Bunkanwanicha and Wiwattanakantang, 2009, and Goldman, Rocholl, and So, 2013).

transitions of insurance commissioners to the private sector and finds evidence of leniency in financial oversight prior to the transition. She further documents that laws that restrict the ability of commissioners to transition to the private sector result in stricter oversight. Using a large sample that includes over 250 agencies, we find no systematic or large-scale evidence of favors provided by executive branch employees to firms while in office.

Second, our results are consistent with those in Cohen (1986), Agarwal, Lucca, Seru, and Trebbi (2014), Lucca, Seru, and Trebbi (2014), and deHaan, Kedia, Koh, and Rajgopal (2015). These papers document that regulatory lenience is associated with a lower proportion of regulators who subsequently switch to the telecommunication and/or financial sector, while aggressive behavior appears to pay off. As we observed earlier, these results are broadly consistent with Che's (1995) signaling model. The evidence in these studies suggests that, at least on average, FCC and/or financial regulators who subsequently move to the industry do not appear to provide systematic favors to the firms they regulate or monitor while at the agency.<sup>8</sup> We provide systematic evidence of aggressiveness on the part of regulators prior to transitioning to the private sector across the largest and most comprehensive set of agencies, industries, and firms, ever examined.

Last but not least, we corroborate the evidence on technical knowledge documented in Shive and Forster (2017) who find that financial firms become less risky after hiring a former financial regulator. They show that this is at least in part due to an increase in risk management activities. Our results provide the first systematic evidence of the knowledge acquired through the revolving door benefiting firms via the allocation of procurement contracts. Moreover, this

---

<sup>8</sup> Outside of government, Kempf (2020) tracks the career paths and credit ratings issued by 245 analysts at Moody's. Consistent with the "knowledge" hypothesis, she documents that, on average, investment banks are more likely to hire more accurate (as opposed to more lenient) analysts. However, consistent with the "quid pro quo" hypothesis, she finds that leniency towards a particular bank increases the likelihood that the analyst lands a job at the bank in question.

knowledge is not detrimental to the government. In fact, firms deliver better on their procurement contracts after hiring former regulators.

Importantly, our results, which are based on very granular data, allow us to greatly mitigate omitted variable concerns. The unit of observation is, in all the specifications, the firm-agency-year triplet, which enables us to include agency-year, firm-year, and firm-agency fixed effects in our specifications, thus leaving little space for possible confounding sources of variation. The use of a dynamic event study methodology further allows us to investigate the precise timing of transitions and associated benefits and distinguish between the “quid pro quo” and the “knowledge” hypotheses.

## 1. Empirical Approach

Throughout our analyses, we will attempt to understand how the revolving door phenomenon arises, where it is most prevalent, and how it is put to use. In addressing these questions, omitted variable concerns represent a non-trivial empirical challenge. The granularity of our data, however, enables us to measure firms’ needs and benefits as narrowly as at the firm-agency-year level. This allows us to greatly mitigate omitted variable concerns through the inclusion of three sets of two-dimensional fixed effects.

We start by investigating the relevance of agency experience to the appointment of former regulators. An intuitive way to do this is to assess whether the appointment of top corporate individuals with experience at agencies correlates with agencies’ actions. We therefore estimate the following model to establish the “relevance” of the revolving door:

$$N. \text{ Former Regulators}_{i,a,t} = \sum_{n=-1}^{+1} \alpha_{t+n} \cdot \text{Restrictions}_{i,a,t+n} + \eta_{i,a} + \zeta_{a,t} + \lambda_{i,t} + \varepsilon_{i,a,t} \quad (1)$$



N. Former Regulators $_{i,a,t}$  is the number of individuals covering top corporate positions at firm  $i$  in year  $t$  with prior work experience at agency  $a$ , i.e., former regulators. We primarily focus on those appointees who are most likely to possess up-to-date technical knowledge and connections. For those individuals, it is also easier to make the case that it is their experience in government, rather than their subsequent experience at another private firm, that gives rise to any observed correlation. To do so, in most of the analyses, we focus on individuals who are appointed by the firm within three years of leaving an agency (see, for example, Cohen (1986)).

The coefficient  $a$  reflects the extent to which the appointment of former regulators is correlated with the regulatory activity of the agency in which the regulator has prior work experience. Restrictions $_{i,a,t}$  is the number of phrases indicating legally binding obligations and prohibitions present in the Code of Federal Regulations (the variable is described in greater detail in section 3.1). We focus on restrictions only in the three years surrounding the transitions from government to top corporate positions after verifying that the inclusion of additional leads and lags does not change the conclusions. It does, however, restrict the sample period we can use.

In those specifications,  $\eta_{i,a}$  are firm-agency fixed effects. These account for any time-invariant firm-agency pair specific omitted variables, such as the proximity between the agency and the firm's headquarters. The coefficient  $a$  consequently isolates how much the appointment or departure of former regulators with experience at agency  $a$  to/from firm  $i$  varies over time as the number of regulations issued by agency  $a$  that apply to firm  $i$  change.

$\zeta_{a,t}$  are agency-year fixed effects. These reflect any time-varying as well as time-invariant agency-specific characteristics, such as staffing, funding, political regimes, propensity to act harshly in general, etc.  $\lambda_{i,t}$  are firm-year fixed effects. These account for any firm-level time-

varying or time-invariant omitted variables. Their inclusion is possible because our analyses include a large set of agencies, thus allowing for numerous firm-agency pairs. Since the model accounts for the specific relationship between each agency and each firm, agency changes over time, and firm changes over time, it is only the variation at the firm-agency-year level that drives the results.

After establishing the relevance of the transitions examined, we turn to the analysis of the possible benefits that can precede or follow the use of the revolving door. For that purpose, we estimate the following dynamic model:

$$Y_{i,a,t} = \sum_{e=-2}^{+2} a_e \cdot \mathbf{1}\{E = e\} + a_6 \cdot \mathbf{1}\{E \geq 3\} + \eta_{i,a} + \zeta_{a,t} + \lambda_{i,t} + \sum_{n=-1}^{+1} b_{t+n} \cdot \text{Restrictions}_{i,a,t+n} + \varepsilon_{i,a,t} \quad (2)$$

which is an event study methodology *à la* Borusyak and Jaravel (2018). The event,  $E$ , is either an increase (“appointment”), or a decrease (“departure”), in the number of former regulators from a given agency appointed to top positions in a firm in a given year (defined in Section 2.1). The unit of observation is, again, the firm-agency-year triplet;  $i$  denotes the firm,  $a$  denotes the agency,  $t$  denotes the calendar year, and  $E$  denotes the time, in years, relative to the event. The benefit of the event study methodology, as opposed to a traditional difference-in-different model, is that it allows for the outcome to *vary* in intensity over the different event years examined.

Importantly, the “time relative to the event” indicators isolate the timing of potential benefits experienced by a firm. This allows us to distinguish between benefits that accrue prior to and following the appointment (or departure) of former regulators, thus distinguishing between the “quid pro quo” and “knowledge” hypotheses. In particular, the “quid pro quo” hypothesis predicts

that firms receive benefits *before* hiring regulators (i.e., former regulators are appointed as compensation for their leniency while in office). The “knowledge” hypothesis predicts that firms benefit *after* hiring executive branch employees, if at all, and possibly following stricter behavior on the part of the regulator. The benefits, if accruing due to the revolving door, should vanish following the departure of former agency employees, and should not be present among individuals that are “out of touch” with the agency, e.g., those that have left the agency several years prior to joining the firm.

$Y_{i,a,t}$  is the “outcome” variable for firm  $i$  in relation to agency  $a$ , in year  $t$  -- where “outcome” is not to be interpreted in a causal sense. In the analyses that follow, the outcomes are measures of fines or procurement contracts. The measures are indicator variables, counts, and dollar values.

Because the events are staggered, and the unit of observation is the firm-agency-year triplet, we are able to include the three sets of fixed effects described above ( $\eta_{i,a}$ ,  $\zeta_{a,t}$ , and  $\lambda_{i,t}$ ) to mitigate potential sources of confounding variation. In these specifications, only one factor could potentially cloud the interpretation of the results: firm-agency pair time-varying omitted variables that correlate with *both* the appointment (or departure) of former regulators and with the “outcome” analyzed. To mitigate this possibility, we control for the “extent” of regulation issued by an agency that applies to firm in a given year, as defined above.

Many of the possible firm-agency pairs are uninformative. Think, for example, of the link between agricultural regulators and firms in finance. The inclusion of such pairs would introduce a large number of uninformative zeros in the panel of data, potentially distorting results. We mitigate this concern by focusing our analyses on the subset of agencies that regulate a firm’s industry (the only exception is the very first regression of Table 3).

## 2. Executive Branch Agency Ties

Our first task is the identification and characterization of the revolving door in executive branch agencies. It is important to first understand *where* it is being used before we ask *why* it is being used. We begin by identifying executive branch agencies and the top corporate individuals who previously worked there. We focus on the flow of personnel from U.S. executive branch agencies because regulations in the U.S. have become increasingly (and are, at present, predominantly) generated by unelected personnel working at these agencies (Matsusaka, 2019).

### 2.1. Data

Our study uses *BoardEx* to classify the career histories of individuals covering top corporate positions in a large sample of 11,957 unique publicly traded and 361,054 unique private U.S. firms during 2002-2018.<sup>9</sup> *BoardEx* is an extensive directory of top corporate individuals (defined in *BoardEx* as “individuals who lead [firms], including board members, C-suite executives and senior leaders”) that contains their career histories, education, executive compensation, and career network. We extract information on the past government experience of each top corporate individual who has work experience in federal executive branch agencies (including the various departments of government) that appear in *BoardEx*. We require each position to have non-missing start and end dates to facilitate the creation of a time series of employment. We then define an individual, working at a given company at a given point in time, as a former regulator from a given agency if they joined the company after working for the regulating agency. We distinguish between two types of former regulators: those who join the firm

---

<sup>9</sup> Frequent “top corporate positions” appearing in *BoardEx* are: Director; Partner; Independent Director; President; Vice President; President/CEO; Chairman; Associate; CEO; CFO; Consultant; Advisor; Senior VP; Manager; Executive VP; Principal; COO; Chairman/COO; Division President.

soon after (i.e., within three years) after leaving the agency, and those who join the firm many years (i.e., more than three) after leaving the agency.

We obtain a list of federal executive branch agencies from the *Federal Register* (<https://www.federalregister.gov/agencies>). While no list of federal executive agencies is officially comprehensive,<sup>10</sup> the *Federal Register* provides the largest list, comprised of 433 such agencies. Our analysis focuses on the 258 executive branch agencies from the *Federal Register* that appear in *BoardEx*.

## 2.2. Summary Statistics

Panel A of Table 1 reports the fraction of firms that have at least one former regulator (from any agency) appointed to a top corporate position. Panel B reports the fraction of firm-agency-year triplets involving firms with former regulators -- the focus of our regression analyses. The averages reported are calculated at the firm-year or firm-agency-year level, respectively, for the full sample period. As shown in the first row of Panel A, 6.5% of firms have at least one former regulator in a top corporate position, and 3.3% of the firms have at least one former regulator in a top corporate position who joined the firm within three years since leaving the agency (i.e., the presumably more relevant “direct transitions”). The data show a large difference in the extent to which public and private firms have former regulators in top corporate positions, with 33.9% of public firms having at least one such individual. This is a much higher fraction relative to the 6.5% reported for the whole sample, which includes both public and private firms. These figures also indicate that the transitions we study in this paper are more prevalent than corporate political connections, lobbying, or campaign contributions (all of which are accounted for in our regression analyses through the inclusion of firm-year fixed effects). The fraction of firms with former regulators in top corporate

---

<sup>10</sup> See [https://www.acus.gov/sites/default/files/documents/Sourcebook%202012%20FINAL\\_May%202013.pdf](https://www.acus.gov/sites/default/files/documents/Sourcebook%202012%20FINAL_May%202013.pdf), pp. 14-15.

positions is also substantially higher among firms with industry classification data available in *Capital IQ*, as well as for relevant (i.e., regulated) firm-agency pairs.

[Insert Table 1 here]

Direct transitions of former regulators who joined a firm within three years since leaving the agency involve individuals with a median (average) work experience at government agencies of four (7.5) years and observed a zero (0.7) year cooling-off period prior to moving to a top position in the private sector (not tabulated). Thus, these are truly “direct” transitions.

Indirect transitions involving former regulators who joined the firm more than three years after leaving government tend to involve individuals with a median (average) work experience at the agency of three (4.2) years and observed a median cooling-off period of 13 (15.4) years prior to joining the firm in question. Thus, not only have these individuals left the agency well before joining the firm on average, questioning the relevance of their experience at the agency, but they also have less experience at the agency.

As shown in Panel C of Table 1, direct transitions of former regulators are predominantly from the Federal Reserve, the Executive Office of the President, the Department of Defense, the Department of State, and the Department of Energy.

The data also enable us to examine how the propensity to appoint former regulators varies across firms in different industries, at least for the subset of firms with available industry classifications. *BoardEx* does not systematically report industry classifications. We therefore use a fuzzy name-matching algorithm to match firms from *BoardEx* to those in *Capital IQ*, from which we can retrieve each firm’s primary six-digit North American Industry Classification System (NAICS) industry code. The industry distribution of the appointment of former regulators is tabulated in Panel D of Table 1. The industry classification used in Panel D of Table 1, for the sake

of conciseness, is the highest NAICS industry level. In terms of direct transitions, the percentage of firms that appoint former regulators is highest among firms operating in the “Utilities,” “Transportation and Warehousing,” and “Educational Services,” industries. Intuitively, it appears that former regulators are more likely to be appointed by firms in industries typically thought of as having closer ties to government.

Figure 1 plots the evolution of the revolving door phenomenon over time across the six broad NAICS industries with the highest incidence of firms with former regulators, as well as, on average, for the rest of the sample. The focus is, once again, on direct transitions. Of the six industries plotted in the table, it is evident that the appointment of former regulators has decreased only among firms in “Educational Services,” especially since 2013. Firms in the “Agriculture, Forestry, Fishing and Hunting” sector experienced a large increase from 2002-2012, although appointments of former regulators have declined among these firms since. The other industries, including “Finance and Insurance” and “Health Care and Social Assistance,” have experienced large increases from 2002-2018.

[Insert Figure 1 here]

Figure 2 provides a visualization of how the appointment of former regulators varies across states. Darker colors indicate a higher fraction of firms headquartered in that state that appointed at least one former regulator directly from an agency. The states with the highest fraction are the District of Columbia (43.9% of the firms), South Dakota (30.3%), and Illinois (29.6%), while Nevada (7.4%), Vermont (5.4%) and New Mexico (2.8%) have the lowest incidence of firms that have former regulators in top corporate positions.

[Insert Figure 2 here]

### 3. Regulations

The data summarized in Table 1 cover individuals who previously worked at executive branch agencies and the firms that subsequently appointed them to top corporate positions. This does not, however, necessarily imply that these individuals are appointed because of their agency experience. To provide a case for the relevance of this experience, we examine the relation between the appointment of former regulators (specifically direct transitions) and the regulations that govern each firm.

#### 3.1. Data

For this purpose, we use a proxy for the extent of regulation as identified by *RegData*,<sup>11</sup> a database containing regulatory data from 1970 through 2019. The proxy, *Restrictions*, is an estimate of the number of phrases indicating legally binding obligations and prohibitions present in the Code of Federal Regulations (CFR). The database is formed using textual analysis to identify regulatory phrases for each part of the CFR. All regulations are published in the CFR, and each agency is given their own portion of the CFR to publish their regulations. *RegData* also uses textual analysis to estimate the relevance of each portion of the CFR to each six-digit NAICS industry, allowing an estimate of regulations at the industry-agency-year level. Table 2 tabulates the average extent of regulation in the 10 most regulated and in the 10 least regulated six-digit NAICS industries during 2002-2018. Industries that heavily employ chemicals are easy to spot among the most regulated.

[Insert Table 2 here]

---

<sup>11</sup> <https://www.quantgov.org/history>



While *RegData* reports the restrictions issued by each agency that apply to each six-digit NAICS industry, *BoardEx* does not systematically report industry classifications. We therefore use a fuzzy name-matching algorithm to match firms from *BoardEx* to those in *Capital IQ*, from which we can retrieve each firm's primary six-digit NAICS industry code. This ensures the widest match between the companies that appear in *BoardEx* and the restrictions in *RegData*. As a consequence of the need to obtain industry classifications from a third source, however, this analysis is only able to include the sub-set of firms covered in *Capital IQ*, resulting in a sample of 32,240,730 relevant firm-agency-year triplets, i.e., those with strictly positive restrictions, and 67,665,918 for the broader sample that includes all possible firm-agency-year combinations with NAICS codes available.

### 3.2. *Regulations and the Revolving Door*

To establish that former regulators are not appointed indiscriminately, we investigate the extent to which more regulation increases the likelihood of affected firms appointing top individuals with experience at the agency in question, focusing on direct transitions. For this purpose, we estimate model (1) using  $\ln(\text{Restrictions}_{i,a,t} + 1)$ , the natural log of the number of phrases indicating legally binding obligations and prohibitions promulgated by agency  $a$  that apply to each six-digit NAICS industry, and thus each firm  $i$ , in year  $t$ , plus one. The sample includes all firms (public and private) with industry-affiliation available in *Capital IQ*.

The results in column (1) of Table 3 include all firms with NAICS data; column (2) includes only agencies that regulate the firm in question in a given year (i.e., with a strictly positive number of restrictions), and column (3) further restricts the sample to fine-imposing agencies that regulate the firm.<sup>12</sup> All specifications include firm-agency, firm-year, and agency-year fixed effects.

---

Note that we do not require that the agency imposed a fine on the firm in question. Rather, we focus on the subset of agencies that imposed a fine on any firm, i.e., on the subset of enforcement agencies.

[Insert Table 3 here]

We start with a sample that includes all agencies, i.e., even those that do not regulate the firm in a given year (or any year), in column (1) of Panel A. We find that former regulators are not only appointed in response to the enactment of new regulations, but also in anticipation of their introduction.

In column (2) of Panel A, we focus on agencies that regulate the firm in a given year. We continue to find evidence supporting the “relevance” of the former regulator’s agency experience. Moreover, the magnitude of the coefficients become greater once we focus on “relevant” agencies, indicating that the high number of firm-agency-year pairs with zero restrictions in column (1) was downward biasing the results. Finally, in column (3) we focus on the sub-set of 41 agencies that impose fines. We find an even higher propensity for firms to appoint a former regulator with recent experience in such agencies at the time of the introduction of new laws and in the year after their introduction. The results show that the appointment of former regulators becomes more common among firms that become more heavily regulated by the agency in question and especially when the new regulations, if violated, may involve a penalty.

While the models reported in the table include only one lead and one lag, we also estimated the regressions including multiple leads and/or lags. The results are generally significant for the leads/lags reported in the table, while additional leads and lags are generally not. That is, the results reported are robust; we do not learn anything additional by including more leads or lags (but, simply, lose observations). We also assess the relevance of agency experience for indirect transitions. In untabulated tests we find that the results, while slightly weaker as expected, are still significant for indirect transitions, suggesting that their experience still holds some relevance.

The results in this section show a significant correlation between the use of the revolving door and where it is relevant.<sup>13</sup> The finding of such robust correlation motivates and justifies the rest of the investigation.

#### **4. Possible Benefits**

Having established that the revolving door phenomenon is more prevalent where it is more relevant, we next investigate our two hypotheses concerning why firms appoint former regulators (the “quid pro quo” and “knowledge” hypotheses). To do so, we focus on two possible benefits: regulatory enforcement (or lack thereof), as proxied by regulatory fines, and procurement contracts. We focus on these two variables because fines are imposed by multiple agencies and, similarly, multiple agencies sign contracts with private contractors. This allows us to mitigate omitted variable concerns through the inclusion the same rich set of fixed effects used in Table 3. The question that we address is whether there is any evidence of an abnormal *reduction* in the incidence of fines or of an *increase* in the signing of procurement contracts in the years that surround the appointment (or departure) of former regulators.

##### *4.1. Regulatory Enforcement*

Financial penalties, or fines, are the most common way that regulatory agencies enforce regulations. The general applicability of fines means that almost any agency can use them to punish firms that violate regulations. Even though each agency may be enforcing different regulations, or punishing otherwise unrelated violations, regulatory fines give us a single measure that captures the extent of enforcement (or lack thereof) across all settings.

---

<sup>13</sup> We reach similar conclusions if we use *Words*, the number of words present in the CFR, or other measures of regulatory complexity.

#### 4.1.1. *Fines: Data*

The data on regulatory fines come from the Corporate Research Project of Good Jobs First’s *Violation Tracker* (<https://www.goodjobsfirst.org/violation-tracker>). The dataset contains regulatory fines issued, since 2000, by more than 41 federal regulatory agencies related to 327,000 civil and criminal cases adding up to more than \$440 billion. We are able to match the fines to 2,257 unique parent firms.

*Violation Tracker* obtains its data through several sources, including the Department of Justice website and the individual agencies’ websites. Many fines are identified using web scraping, but the data are also supplemented using Freedom of Information Act requests. Fines below \$5,000 are not reported in *Violation Tracker*, as well as penalties with no dollar amount (such as some issued by the FDA, only requiring companies to suspend sales of dangerous products). *Violation Tracker* matches entities to a *current* parent company, even if they were not acquired until after the penalty was imposed. Our analysis focuses on the 41 agencies that appear in both *BoardEx* and *Violation Tracker*. Table 4 presents summary statistics of the fines that comprise our sample.

[Insert Table 4 here]

#### 4.1.2. *Fines: Results*

With data on regulatory fines, we investigate whether the appointment (or departure) of former regulators is associated with a change in the incidence of fines. We estimate a linear probability model of the relation between regulatory enforcement and the appointment of former regulators. The model uses the event-study structure of model (2) discussed in Section 1.

The event is either an increase (“appointment”), or a decrease (“departure”) in the number of former regulators at the firm. The unit of observation is the firm-agency-year triplet. As before,

$i$  denotes the firm,  $a$  denotes the agency,  $t$  denotes the calendar year, and  $E$  denotes the time relative to the event.

In the Table 5 regressions, each firm is paired with each of the 41 federal regulatory agencies covered in the *Violation Tracker* database that could be matched to *BoardEx*, resulting in a panel of 6,916,855 firm-agency-year observations. In columns (1) and (3), the dependent variable,  $Fines_{i,a,t}$ , is an indicator that takes the value of one if a fine is imposed on firm  $i$  by agency  $a$  during year  $t$ , and zero otherwise.<sup>14</sup> In columns (2) and (4), the dependent variable,  $\ln(Fines\ Amount+1)_{i,a,t}$ , is the natural log of the actual amount of the fine(s) plus one.

Columns (1) and (2) present the results for “appointments,” i.e., increases in the number of former regulators appointed to top corporate positions. Columns (3) and (4) present the results for “departures” of such regulators, i.e., decreases in the number of former regulators that occupied top corporate positions. Panel A of Table 5 focuses on direct transitions, i.e., top corporate individuals who joined the firm within three years since leaving the agency.

The “quid pro quo” hypothesis predicts that firms appoint former regulators in exchange for their leniency while in office, i.e., abnormally low fines prior to the hiring. The results that are relevant for the “quid pro quo” hypotheses are therefore those concerning the incidence (or amount) of fines in each of the two years that precede the appointment, as well as the year of the transition. Contrary to the predictions of the “quid pro quo” hypothesis, in no regression are those coefficients negative and significant. That is, we find no evidence of a decline in the magnitude of fines in the years that immediately precede the appointment of former regulators. In fact, we find that the hiring of former regulators tends to coincide with *more* frequent and abnormally high fines. The economic magnitude of the coefficient is very large: firms experience an abnormal incidence

---

Note that the sample includes both firms that received fines as well as those that never received a fine.

of fines in the measure of 1.8% in the year of the appointment of former regulators; this compares to an average incidence of fines across all firm-agency-years of 0.1%. Thus, firms appear to hire regulators in immediate response to, or anticipation of, an abnormally (and meaningfully) high incidence of fines.

The result that former regulators are, at least on average, appointed in conjunction with harsh treatment by executive branch agencies is in line with prior evidence in Cohen (1986), Agarwal et al. (2014), Lucca et al. (2014), and deHaan et al. (2015); it is also consistent with Che's (1995) signaling model in which regulators seeking a job in the industry signal their quality through aggressive behavior.

We also consider the years that follow the transition, in particular the two years that immediately follow it, as well as the years after collectively. Those years are relevant for testing a version of the "knowledge" hypothesis in which firms obtain benefits *ex-post*, either due to the technical or to the connections knowledge of former regulators. While none of those coefficients are significant, the appointment of the regulator is followed by fines returning to "normal" levels.

We next exploit "departures" to investigate the notion that fines returning to pre-transition levels might be the result of knowledge brought in by the former regulator. If the return of fines to "normal" (i.e., pre-shock) levels occurs because of the appointment of a former regulator, the fines should increase once the former regulator departs. That is, since the departure of the former regulator coincides with the loss of technical or connections knowledge, fines should spike *ex-post*. Columns (3) and (4) investigate whether this is the case. The results show an abnormally high incidence of fines, albeit only temporarily, after the departure of the former regulator, specifically in the second year after her departure. This result is also consistent with the "knowledge" hypothesis, and as with appointments, the economic magnitude of the abnormal

increase in the incidence of fines that follows the departure of a former regulator is high. In the second year that follows the departure of former regulators, firms experience an abnormal incidence of fines measured at 1.6%; once again compared to an average incidence of fines across all firm-agency-years of 0.1%.

Could it be that these results are spurious in that, out of many coefficients, some will be significant just by chance? To investigate this possibility, as a placebo, we consider the pattern of fines around the appointment and the departure of regulators that have less current technical and/or connections knowledge because they left the agency several years (more than three -- recall that the median is 15) before joining the firm. Panel B of Table 5 presents these results. For indirect transitions we find a rather symmetric incidence of fines around both the appointment as well as the departure of former regulators. Importantly, indirect transitions do not appear to occur concomitant with the imposition of fines. Thus, the strongest phenomenon in Panel A, the likely endogenous contemporaneous correlation between fines and appointments, is completely absent in Panel B. This suggests that the results in Panel A are a function of immediately relevant and/or current knowledge that individuals who left an agency a long time before transitioning to the firm no longer possess.

[Insert Table 5 here]

#### 4.2. *Procurement Contracts*

While the results for fines do not provide clear evidence of benefits to firms, at least on average, they do further highlight the relevance of the agency in explaining transitions. Firms certainly appear to respond to agency-related shocks. But we still do not see a clear picture of benefits. To investigate this further, we examine a second type of benefit that the revolving door might bring to the firm: procurement contracts.

#### 4.2.1. Procurement Contracts: Data

We obtain data on procurement contracts from *USASpending.gov*. The dataset contains contracts signed by 197 federal agencies that can be matched to firms in *BoardEx*. We use these data to construct three measures of contracting with the US government. *Contract Dummy* is an indicator variable that takes the value of one if the agency in question signs a contract with the firm in a given year, and 0 otherwise.  $\ln(N \text{ Contracts} + 1)$  is the natural log of the number of contracts the agency in question signs with the firm in a given year, plus one.  $\ln(\text{Contract Value} + 1)$  is the natural log of the total “Federal Action Obligation” across all contracts the agency in question signs with the firm in a given year. The sample includes all government contracts but excludes contract IDVs, grants, direct payments, loans, insurance, sub awards, and other financial assistance. We remove contracts with a missing or negative “federal action obligation” value, and match to firms in *BoardEx* using the same fuzzy string-matching algorithm used in the previous section.

#### 4.2.2. Procurement Contracts: Results

We use the procurement contracts data to investigate whether the appointment (or departure) of former regulators, in particular direct transitions, is associated with a change in the frequency (or amount) of procurement contracts. As with fines, we use the event-study structure of model (2) discussed in Section 1. In Panels A and B of Table 6, the dependent variable is: *Contract Dummy* in columns (1) and (4);  $\ln(N \text{ Contracts} + 1)$  in columns (2) and (5); and  $\ln(\text{Contract Value} + 1)$  in columns (3) and (6).

[Insert Table 6 here]

The “quid pro quo” hypothesis predicts a significantly higher incidence, number, or value of procurement contracts prior to the direct transition of the former regulator to the private sector



-- recall the Druyun case. The “knowledge” hypothesis predicts a higher incidence, number, or value of contracts following the appointment of former regulators. As with fines, the results in Panel A of Table 6 do not provide significant evidence in support of the “quid pro quo” hypothesis. None of the event study dummies in columns (1)-(3) for the event years -2, -1, or zero are statistically significant (and positive). Instead, the event study shows evidence of an abnormally high incidence, number, and value of contracts *following* the appointment of former regulators, especially in the year that follows their appointment. This result is consistent with a version of the “knowledge” hypothesis in which firms either benefit from the current technical or connections knowledge of the former regulator.

In columns (4)-(6), we again use departures to investigate whether the documented result is spurious. It appears not to be. The only coefficients that are significant are negative, and indicate an abnormally lower incidence of procurement contacts following the departure of former regulators -- a result that is also in line with the predictions of the “knowledge hypothesis”.

In Panel B we focus once again on the placebo test involving regulators that left the agency more than three years prior to joining the firm. We find limited evidence of an abnormally high incidence of contracts two years before their hiring (column (1)). An inattentive interpretation of this result would be to support the “quid pro quo” hypothesis. Such interpretation is necessarily incorrect as, in Panel B, the former regulators we consider were no longer employed in government at that time. A more plausible interpretation is that such result is spurious. The appointment of former regulators that left the agency several years before joining the firm is not associated with significant benefits, in terms of procurement contracts, subsequent to their hiring. We find no

evidence of a lower incidence of procurement contracts after the departure of this set of former regulators.<sup>15</sup>

While the results for both procurement contracts and fines are so far consistent with the “knowledge” hypothesis, they are silent on whether they are a function of *what* or *who* the former regulator knows. While the data does not allow us to completely distinguish between the two, it does allow us to address a related and perhaps more important question: does the knowledge brought to the firm by former regulators distort the allocation of procurement contracts to the benefit of the worst firms?

To address this question, we follow Schoenherr (2019) and rely on the assumption that, if knowledge was distorting the allocation of contracts to the benefit of worse firms, we should find evidence of contracts being executed systematically worse. This can be observed through systematic price-increasing revisions for contracts signed under the guidance of former regulators. We therefore examine the probability of a contract being renegotiated conditional on the number of former regulators appointed to top corporate positions at the firm.

In this test, the unit of observation is each individual contract signed between a firm and an agency. The dependent variable, *Reneg Dummy*, is an indicator denoting whether a signed contract experienced a price increase at any point in time before its completion. Our main variable of interest, *Direct Transitions (t-1)*, is an indicator for whether a former regulator was appointed (direct transition) the year before the contract was signed. We also control for the restrictions promulgated by the agency that affect the firm in the year the contract is signed, the initial value of the contract, and the number of offers received by the agency for the contract during the bidding

---

<sup>15</sup> For both fines and procurement contracts, we reach similar conclusions when we progressively include the various fixed effects. That is, while the evidence supports the knowledge hypothesis, we find no systematic evidence of *quid pro quo*.

process. If the number of offers is not reported, we set it equal to one and set an indicator, N Offers Missing, equal to one. Given that the vast majority of firms sign many contracts with the same agency, we are able to include the same sets of fixed effects used in previous tests, to which we add award-type fixed effects.<sup>16</sup>

[Insert Table 7 here]

The results for this test are reported in column (1) of Table 7. Interestingly, we find no evidence of the presence of former regulators increasing the probability of renegotiation. In fact, contracts signed following the appointment of former regulators to top corporate positions are less likely to be renegotiated. Thus, in terms of price-increasing revisions, the appointment of former regulators is associated with *better* contract execution.

In column (2), we investigate whether the appointment of former regulators is associated with a higher increase in price when a contract does end up being renegotiated. Conditioning on renegotiation, we aggregate all changes in negotiated value over the life of the contract and divide by the initial obligation to calculate the renegotiation percent (i.e., if a \$1m contract is renegotiated to \$1.1m,  $\text{Reneg \%} = (1.1\text{m} - 1\text{m}) / 1\text{m}$ ). We use this quantity as the dependent variable in column (2) of Table 7, but still do not find a statistically significant, positive relationship between contract renegotiation and the appointment of former regulators. Overall, the evidence in Table 7 rejects the idea that the revolving door is systematically used to abuse knowledge to distort contracts to the benefit of firms that would deliver a worse (or more expensive) product.

---

Award types include government-wide agency contracts approved by the Office of Management and Budget, indefinite delivery contracts, General Services Administration or Veterans Affairs federal supply schedules, basic ordering agreements, and blanket purchase agreements.

## 5. Then Why The “Quid Pro Quo” Concerns?

Is the criticism of the revolving door therefore largely unjustified? Most of the empirical evidence systematically supporting the “quid pro quo” view of the revolving door (e.g., Fisman, 2001, Khwaja and Mian, 2005, Faccio *et al.*, 2005, Faccio, 2006, Bunkanwanicha and Wiwattanakantang, 2009, and Goldman et al., 2013) actually focuses on powerful elected politicians such as Presidents, Prime Ministers, or members of important regulatory committees who have sufficient power to benefit their cronies. At the same time, with a few exceptions, studies involving transitions of unelected employees of government agencies to the private sectors rarely find evidence of abuse of power for personal gain. It could well be that many of the agency employees do not have sufficient discretionary power to influence important decisions. It could also be that they do not have sufficient incentives. Many agency employees may also prefer a more secure job in government to a highly risky corporate career.

Nonetheless, incentives are likely to change over time. In particular, a non-trivial number of agency employees, specifically Presidential appointees, are replaced at the end of each President’s term. Individuals expecting their replacement might attempt to facilitate a transition to the private sector by behaving leniently towards the industry immediately prior to their expected replacement, i.e., leading up to a Presidential election.<sup>17</sup> In Table 8 we investigate whether that is the case. In particular, we compare the incidence of fines and contracts for direct transitions that occur in the year that immediately follows a Presidential election, where incentives to benefit an industry might be highest, to transitions that occur in other years. As before, we exploit the dynamic structure of our panel in an event study setting.

---

<sup>17</sup> This is especially true in the case of a different political party winning the Presidency. For example, while both President Clinton and President Trump signed executive orders creating mandated cooling-off periods for presidential appointees, both presidents revoked their orders before leaving office, effectively allowing their appointees to directly transition to industry jobs.

[Insert Table 8 here]

Panel A of Table 8 presents the results for fines. Panel B presents the results for procurement contracts. Consistent with the “quid pro quo” view of the revolving door, the results of Panel A show an abnormally low amount of fines in the year prior to a transition that occurs immediately following a Presidential election. The incidence of fines is also lower immediately prior to a transition, although the results lack significance at traditional confidence levels. Panel B shows evidence of an abnormally high incidence of procurement contracts, as well as an abnormally high value, in the year immediately prior to transitions that occur in the year that follows a Presidential election. It appears that a shock that is likely to increase the chance of termination in government, at least for some, leads to behavior that is pro-industry. Thus, although limited to specific years, we document some evidence consistent with the “quid pro quo” view of the revolving door.

## **6. Conclusion**

The revolving door between government and the private sector often draws criticism from the media, as well as from academics. However, there has been no systematic documentation of the phenomenon. In this paper, we provide the first comprehensive database tracking flows of personnel from over 250 federal executive branch agencies (in the U.S.) to top corporate positions. We show that the revolving door phenomenon is indeed pervasive, especially among larger firms, and a substantial fraction of those flows are direct transitions from an agency to a firm.

We document that the agency experience of former regulators is highly relevant for their appointment to top corporate positions. We find that former regulators tend to be appointed in response to the enactment of new regulations or concomitant with an increase in the incidence of fines.

As to why firms appoint former regulators to top positions, the results are predominantly consistent with the “knowledge” hypothesis. Firms tend to appoint former regulators from agencies that acted harshly toward them. Furthermore, firms benefit *following* the appointment of former regulators, and lose these benefits following the departure of former regulators. Placebo tests exploiting regulators that are less likely to possess current knowledge or connections show no such pattern, thus indicating that the results are closely tied to the immediately relevant nature of the former regulator’s knowledge. Importantly, these results are present in an event study setting that allows examination of the exact timing of possible benefits and in an econometrically stringent setting that includes firm-year, agency-year, and firm-agency fixed effects, leaving little space for confounding sources of variation.

While our large-scale evidence does not support the “quid pro quo” view of the revolving door, we do uncover some evidence consistent with quid pro quo among transitions that occur immediately following presidential elections. This evidence is only suggestive, and data limitations prevent us from making stronger conclusions in this setting. Nonetheless, we believe the pattern of benefits observed prior to transitions that follow presidential elections certainly deserves more attention. We hope that future studies, perhaps exploiting data that are not available to the public at present, will be able to analyze this issue in greater detail.

## References

- Agarwal, Sumit, David Lucca, Amit Seru, and Francesco Trebbi, 2014, Inconsistent regulators: Evidence from banking, *Quarterly Journal of Economics* 129, 889-938.
- Bertrand, Marianne, Matilde Bombardini, and Francesco Trebbi, 2014, Is it whom you know or what you know? An empirical assessment of the lobbying, *American Economic Review* 104, 3885-3920.
- Bien, Jeffrey, and Vinay Prasad, 2016, Future jobs of US Food and Drug Administration's hematology-oncology medical reviewers, *BMJ*; 354 doi: <https://doi.org/10.1136/bmj.i5055>.
- Blanes i Vidal, Jordi, Mirko Draca, and Christian Fons-Rosen, 2012, Revolving door lobbyists, *American Economic Review* 102, 3731-3748.
- Borusyak, Kirill, and Xavier Jaravel, 2018, Revisiting event study designs, with an application to the estimation of the marginal propensity to consume, working paper, Harvard University and the London School of Economics.
- Bunkanwanicha, Pramuan and Yupana Wiwattanakantang, 2009, Big business owners in politics, *Review of Financial Studies* 22, 2133-2168.
- Che, Yeon-Koo, 1995, Revolving doors and the optimal tolerance for agency collusion, *RAND Journal of Economics* 26, 378-397.
- Cohen, Jeffrey E., 1986, The dynamics of the "revolving door" on the FCC, *American Journal of Political Science* 30, 689-708.
- Cornaggia, Jess, Kimberly J. Cornaggia, and Han Xia, 2016, Revolving doors on Wall Street, *Journal of Financial Economics* 120, 400-419.
- deHaan, Ed, Simi Kedia, Kevin Koh, and Shivaram Rajgopal, 2015, The revolving door and the SEC's enforcement outcomes: Initial evidence from civil litigation, *Journal of Accounting and Economics* 60, 65-96.

- Faccio, Mara, 2006, Politically connected firms, *American Economic Review* 96, 369-386.
- Faccio, Mara, Ronald W. Masulis, and John J. McConnell, 2005, Political connections and corporate bailouts, *Journal of Finance* 61, 2597-2635.
- Fisman, Raymond, 2001, Estimating the value of political connections, *American Economic Review* 91, 1095-1102.
- Goldman, Eitan, Jorg Rocholl, and Jongil So, 2013, Political connections and the allocation of procurement contracts, *Review of Finance* 13, 1617-1648.
- Gormley, William T., 1979, A test of the revolving door hypothesis at the FCC, *American Journal of Political Science* 23, 665-683.
- Haselmann, Rainer, David Schoenherr, and Vikrant Vig, 2021, Rent-seeking in elite networks, *Journal of Political Economy*, forthcoming.
- Kempf, Elisabeth, 2020, The job rating game: The effects of revolving doors on analyst incentives, *Journal of Financial Economics* 135, 41-67.
- Khwaja, Asim Ijaz, and Atif Mian, 2005, Do lenders favor politically connected firms? Rent-seeking in an emerging financial market, *Quarterly Journal of Economics* 120, 1371-1411.
- Lucca, David, Amit Seru, and Francesco Trebbi, 2014, The revolving door and worker flows in banking regulation, *Journal of Monetary Economics* 65, 17-32.
- Matsusaka, John G., 2019, *Democracy Adrift: Populism, Referendums, and the Quest for Self-Government*.
- Peltzman, Sam, 1976, Toward a more general theory of regulation, *Journal of Law and Economics* 19, 211-240.
- Sapienza, Paola, 2004, The effects of government ownership on bank lending, *Journal of Financial Economics* 72, 357-384.



- Schoenherr, David, 2019, Political connections and allocative distortions, *Journal of Finance* 74, 543-586.
- Shive, Sophie A., and Margaret M. Forster, 2017, The revolving door for financial regulators, *Review of Finance* 21(4), 1445-1484.
- Stigler, George J., 1971, The theory of economic regulation, *The Bell Journal of Economics and Management Science* 2, 3-21.
- Tabakovic, Haris, and Thomas Wollmann, 2018, Effects of regulatory capture: evidence from patent examiners, working paper, Harvard Business School and University of Chicago Booth.
- Tenekedjieva, Ana-Maria, 2020, The revolving door and insurance solvency regulation, working paper, University of Chicago Booth.

Table 1. Prevalence of the Revolving Door

The table provides averages of the prevalence of the revolving door, i.e., the appointment of former regulators to top corporate positions, in the U.S. during 2002-2018. The appointment of former regulators is measured either at the company-year level (Panels A, C, and D) or at the company-agency-year level (Panel B). In the former case the Table reports, for the panel, the percentage of firms with at least one individual in a top corporate position with work experience in at least one of 258 executive branch agencies from the *Federal Register*. In the latter case, the Table reports, for the panel, the percentage of individuals covering top corporate positions who have work experience at the agency in question. *Public* firms are labeled in *BoardEx* as “Quoted.” Panel A reports the percentage of firm-years who have individuals in top corporate positions with work experience in at least one of 258 executive branch agencies from the *Federal Register*. Panel B reports the percentage of firm-agencies-years who have individuals in top corporate positions with prior work experience at that agency. Direct transitions involve individuals who were appointed to a top position in the firm within three years since leaving the agency; Indirect transitions involve individuals who are appointed to a top position in the firm more than three years since leaving the agency. Panel C reports statistics by agency, while Panel D reports statistics by NAICS industry, whenever available in *Capital IQ*.

Panel A: Full Sample, *Firm-Year Pairs (%)*

	<b>N. Obs.</b>	<b>Total</b>	<b>Direct Trans.</b>	<b>Indirect Trans.</b>
<b>All</b>	<b>3,024,282</b>	<b>6.55%</b>	<b>3.28%</b>	<b>4.51%</b>
<i>of which: Public</i>	<i>106,053</i>	<i>33.94%</i>	<i>17.40%</i>	<i>27.35%</i>
w/ NAICS Codes	262,271	18.59%	8.98%	14.53%
Res > 0	186,555	19.20%	9.04%	14.97%
Res > 0, Fine Imposing	186,555	10.10%	3.45%	8.03%
Res > 0, Contracts	186,555	16.00%	6.09%	13.13%

Panel B: Full Sample, *Firm-Agency-Year Triplets (%)*

	<b>N. Obs.</b>	<b>Total</b>	<b>Direct Trans.</b>	<b>Indirect Trans.</b>
<b>All</b>	<b>780,264,756</b>	<b>0.05%</b>	<b>0.02%</b>	<b>0.03%</b>
<i>of which: Public</i>	<i>27,361,674</i>	<i>0.31%</i>	<i>0.11%</i>	<i>0.21%</i>
w/ NAICS Codes	67,665,918	0.15%	0.05%	0.10%
Res > 0	32,240,730	0.22%	0.08%	0.15%
Res > 0, Fine Imposing	6,916,855	0.38%	0.11%	0.29%
Res > 0, Contracts	22,024,379	0.25%	0.08%	0.18%

Panel C: Agency Distribution of Former Regulators

	Firm-Year %; Direct Trans.	
	All Firms	Public Firms
Federal Reserve	2.98%	6.91%
Executive Office of the President	1.09%	2.72%
Department of Defense	0.59%	1.35%
Department of State	0.56%	1.34%
Department of Energy	0.44%	0.97%
Securities and Exchange Commission	0.41%	0.89%
Food and Drug Administration	0.41%	0.77%
National Science Foundation	0.40%	0.85%
Department of Commerce	0.37%	0.85%
Department of the Treasury	0.34%	0.76%
Department of Justice	0.29%	0.72%
Department of Health and Human Services	0.27%	0.57%
National Aeronautics and Space Administration	0.26%	0.62%
Federal Communications Commission	0.24%	0.60%
Department of Veterans Affairs	0.24%	0.45%
...	...	...

Panel D: Industry Distribution of Former Regulators

NAICS Industry	N	Direct Trans.
Utilities	7,187	19.7%
Transportation and Warehousing	5,711	14.7%
Educational Services	1,190	13.6%
Agriculture, Forestry, Fishing and Hunting	648	13.6%
Health Care and Social Assistance	5,084	12.5%
Finance and Insurance	56,628	11.8%
Professional, Scientific, and Technical Services	11,667	10.0%
Public Administration	48	8.3%
Manufacturing	81,547	8.1%
Construction	3,420	7.5%
Accommodation and Food Services	4,138	7.5%
Retail Trade	8,473	7.4%
Information	34,190	6.9%
Administrative and Support and Waste Mgmt and Rem. Services	5,033	6.9%
Real Estate Rental and Leasing	9,595	6.9%
Wholesale Trade	7,219	6.5%
Other Services (except Public Administration)	965	4.7%
Mining, Quarrying, and Oil and Gas Extraction	16,843	4.6%
Arts, Entertainment, and Recreation	2,548	4.3%
Unclassified	120	1.7%
Management of Companies and Enterprises	17	0.0%

Table 2: Industry Distribution of Regulations

The Table reports the 10 most and 10 least regulated six-digit NAICS industries based on the average number of restrictions during 2001-2019. *Restrictions* is an estimate of the number of phrases indicating legally binding obligations and prohibitions present in the Code of Federal Regulations (CFR). The variable is from *RegData*, a database that uses textual analysis to identify regulatory phrases for each part of the CFR.

<b>NAICS Descriptions</b>	<b>NAICS Code</b>	<b>Restrictions</b>
Gum and Wood Chemical Manufacturing	325191	75,313
Inorganic Dye and Pigment Manufacturing	325131	72,220
Cyclic Crude and Intermediate Manufacturing	325192	70,333
Alkalies and Chlorine Manufacturing	325181	69,507
Polish and Other Sanitation Good Manufacturing	325612	69,426
Synthetic Dye and Pigment Manufacturing	325130	69,236
Paint and Coating Manufacturing	325510	68,883
Plastics Material and Resin Manufacturing	325211	68,481
Medicinal and Botanical Manufacturing	325411	66,389
Ethyl Alcohol Manufacturing	325193	66,176
...	...	...
Other Structural Clay Product Manufacturing	327123	198
Linen Supply	812330	197
Other Communication and Energy Wire Manufacturing	335929	195
All Other Miscellaneous Textile Product Mills	314999	194
Other Lighting Equipment Manufacturing	335129	191
Offices of Real Estate Agents and Brokers	531210	182
Commercial Printing (except Screen and Books)	323111	179
Bare Printed Circuit Board Manufacturing	334412	175
Clay Building Material and Refractories Manufacturing	327120	167
Brick and Structural Clay Tile Manufacturing	327121	155

Table 3. Regulations and the Revolving Door

The unit of observation is the firm-agency-year triplet. The dependent variable,  $N. Former Regulators$ , is the number of individuals covering top corporate positions with work experience in agency  $a$ . The analyses focus on direct transitions.  $Restrictions$  is an estimate of the number of phrases indicating legally binding obligations and prohibitions present in the Code of Federal Regulations (CFR). The variable is from  $RegData$ , which uses textual analysis to estimate the relevance of each portion to each six-digit NAICS industry, allowing an estimate of regulations at the industry-agency-year level. The sample includes firms with primary 6-digit NAICS industry code available in  $Capital IQ$ . Column (1) includes all possible firm-agency-year triplets, Column (2) only includes firm-agency-year triplets with a strictly positive number of restrictions; Column (3) further restricts the sample to the 41 agencies for which we have data on the fines they imposed. T-stats based on standard errors clustered at the firm-agency level are reported in parentheses below the coefficients.

	(1)	(2)	(3)
$\ln(\text{Restrictions} + 1) [t-1]$	0.000115*** (3.90)	0.000186*** (3.71)	0.000257** (2.52)
$\ln(\text{Restrictions} + 1) [t]$	0.000024 (0.94)	0.000128** (2.13)	0.000380*** (2.91)
$\ln(\text{Restrictions} + 1) [t+1]$	0.000104** (2.46)	0.000136 (1.54)	-0.000031 (-0.13)
N. Obs.	67,665,918	32,240,730	6,916,855
Adj. R <sup>2</sup>	0.822	0.676	0.565
SE Cluster	Firm-Agency	Firm-Agency	Firm-Agency
Firm-Agency FEs	Y	Y	Y
Agency-Year FEs	Y	Y	Y
Firm-Year FEs	Y	Y	Y
Y Time Period	2002-2018	2002-2018	2002-2018
Sample	w/ NAICS Code	Res > 0	Res > 0, Fine Imposing
Y Sample Mean	0.0006212	0.0008746	0.0011907

Table 4. Fines by Agency (in Thousands of Dollars).

This table reports, by agency, the fines imposed on firms that could be matched with *BoardEx*. Fines are from the Corporate Research Project of Good Jobs First's *Violation Tracker* (<https://www.goodjobsfirst.org/violation-tracker>).

<b>Agency</b>	<b>N. of Fines</b>	<b>Mean</b>	<b>Median</b>	<b>Min</b>	<b>Max</b>
Occupational Safety & Health Administration	2,423	40	14	5	6,050
Environmental Protection Agency	1,256	3,664	49	5	1,013,400
Department of Labor	728	507	35	5	84,000
National Labor Relations Board	528	462	30	5	97,183
Federal Railroad Administration	460	90	12	5	2,006
Department of Justice	316	276,955	16,800	9	13,000,000
Federal Aviation Administration	285	485	41	5	27,441
Mine Safety & Health Administration	278	175	41	5	3,103
Securities and Exchange Commission	230	43,907	6,550	25	800,000
Equal Employment Opportunity Commission	119	1,035	150	5	24,000
Federal Motor Carrier Safety Administration	72	19	10	5	117
Pipeline and Hazardous Materials Safety Administration	68	154	89	14	739
Bureau of Industry and Security	67	31,788	115	6	1,400,000
Office of the Comptroller of the Currency	64	122,765	3,000	5	2,318,450
Office of Foreign Assets Control	54	14,285	24	5	329,594
Department of Transportation	49	328	90	20	2,785
Commodity Futures Trading Commission	45	38,220	1,500	25	425,000
Centers for Medicare & Medicaid Services	43	532	165	14	3,101
Federal Energy Regulatory Commission	42	20,996	3,610	80	410,000
Consumer Financial Protection Bureau	36	156,797	32,500	34	2,125,000
Federal Communications Commission	35	1,422	368	10	13,376
Federal Trade Commission	31	26,258	3,500	250	280,000
Nuclear Regulatory Commission	22	309	60	10	5,450
Consumer Product Safety Commission	21	2,493	950	45	27,250
Food and Drug Administration	21	341,105	55,000	550	2,201,200
Housing and Urban Development Department	18	12,410	663	11	200,000
Federal Deposit Insurance Corporation	17	14,602	30	5	140,000
Health & Human Services Department Office of Inspector General	15	984	353	18	4,678

Department of the State	14	12,821	8,000	3,000	55,000
Department of Energy	11	196	135	8	578
Drug Enforcement Administration	7	36,919	34,000	834	80,000
Bureau of Safety and Environmental Enforcement	4	79	25	15	250
Alcohol and Tobacco Tax and Trade Bureau	4	1,050	243	14	3,700
Department of the Interior	3	4,671	1,208	566	12,240
National Highway Traffic Safety Administration	3	15,250	9,000	1,750	35,000
Federal Housing Finance Agency	2	1,630,000	1,630,000	110,000	3,150,000
National Credit Union Administration	2	36,750	36,750	20,500	53,000
Treasury Department Financial Crimes Enforcement Network	2	265,500	265,500	70,000	461,000
U.S. Fish and Wildlife Service	2	1,750	1,750	1,000	2,500
Federal Maritime Commission	1	200	200	200	200

---

Table 5. Enforcement Actions and the Revolving Door: Fines

The unit of observation is the firm-agency-year triplet. The dependent variable in Columns (1) and (3), *Fines*, is an indicator taking the value of one if a fine is imposed on firm *i* by agency *a* during year *t*, and zero otherwise. Fines are from the Corporate Research Project of Good Jobs First's *Violation Tracker* (<https://www.goodjobsfirst.org/violation-tracker>). The dependent variable in Columns (2) and (4),  $\ln(\text{Fines Amount}+1)$ , is the natural log of the amount of fines imposed on firm *i* by agency *a* during year *t*, plus one. In Columns (1) and (2) the events analyzed are appointments of former regulators to top corporate positions, while in Columns (3) and (4) the events considered consists of departures of former regulators from the firm. *Event Year = t* denotes the time relative to appointment/departure event, in years. Panel A includes transitions to a top corporate position (i.e., the Event) that occur within three years since the individual leaves government. Panel B includes transitions to top corporate positions involving former regulators that join the firm more than three years since leaving government. The sample includes both public and private firms. T-stats based on standard errors clustered at the firm-agency level are reported in parentheses below the coefficients.

Panel A: Direct Transitions

Event Type:	Appointment of Former Regulator		Departure of Former Regulator	
	Fines Dummy (1)	$\ln(\text{Fines Amount}+1)$ (2)	Fines Dummy (3)	$\ln(\text{Fines Amount}+1)$ (4)
Event Year = -2	0.000758 (0.16)	-0.006526 (-0.09)	0.007169 (1.34)	0.110453 (1.33)
Event Year = -1	0.000335 (0.07)	-0.015613 (-0.22)	0.004198 (0.87)	0.064022 (0.85)
Event Year = 0	0.018372*** (3.17)	0.274404*** (3.10)	-0.000342 (-0.07)	-0.020051 (-0.25)
Event Year = +1	0.004732 (0.94)	0.057888 (0.75)	0.008035 (1.30)	0.122201 (1.28)
Event Year = +2	0.004424 (0.92)	0.065091 (0.89)	0.015943** (2.34)	0.236350** (2.22)
Event Year $\geq$ +3	0.006311 (1.56)	0.072590 (1.22)	0.004180 (0.99)	0.047503 (0.69)
$\ln(\text{Restrictions} + 1)$ [t-1]	0.000072 (0.69)	0.001522 (1.17)	0.000073 (0.70)	0.001547 (1.19)
$\ln(\text{Restrictions} + 1)$ [t]	-0.000041 (-0.25)	-0.000169 (-0.09)	-0.000041 (-0.25)	-0.000168 (-0.09)
$\ln(\text{Restrictions} + 1)$ [t+1]	0.000141	0.002177	0.000145	0.002218



	(0.91)	(1.14)	(0.93)	(1.16)
N. Obs.	6,916,855	6,916,855	6,916,855	6,916,855
Adj. R <sup>2</sup>	0.284	0.265	0.284	0.265
Cluster	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency
Firm-Agency FEs	Y	Y	Y	Y
Agency-Year FEs	Y	Y	Y	Y
Firm-Year FEs	Y	Y	Y	Y
Y Time Period	2002-2018	2002-2018	2002-2018	2002-2018
Sample	Res > 0, Fine Imposing	Res > 0, Fine Imposing	Res > 0, Fine Imposing	Res > 0, Fine Imposing
Y Sample Mean	0.0010693	0.0118394	0.0010693	0.0118394

Panel B: Indirect Transitions

Event Type:	Appointment of Former Regulator		Departure of Former Regulator	
	Fines Dummy (1)	ln(Fines Amount+1) (2)	Fines Dummy (3)	ln(Fines Amount+1) (4)
Event Year = -2	0.008405** (2.52)	0.125931** (2.47)	0.001514 (0.49)	0.021157 (0.44)
Event Year = -1	0.000866 (0.31)	0.030863 (0.67)	-0.001067 (-0.33)	-0.007414 (-0.14)
Event Year = 0	0.002845 (1.04)	0.048946 (1.15)	-0.000928 (-0.33)	-0.020826 (-0.47)
Event Year = +1	0.001506 (0.53)	0.038604 (0.88)	-0.000520 (-0.16)	-0.003983 (-0.08)
Event Year = +2	0.006113* (1.93)	0.121329** (2.35)	-0.001648 (-0.51)	-0.024505 (-0.50)
Event Year >= +3	0.001707 (0.77)	0.039109 (1.13)	-0.001067 (-0.38)	-0.008841 (-0.21)
ln(Restrictions + 1) [t-1]	0.000073 (0.70)	0.001532 (1.18)	0.000073 (0.70)	0.001536 (1.18)
ln(Restrictions + 1) [t]	-0.000038	-0.000123	-0.000037	-0.000116

	(-0.23)	(-0.06)	(-0.23)	(-0.06)
ln(Restrictions + 1) [t+1]	0.000145	0.002170	0.000152	0.002288
	(0.93)	(1.13)	(0.98)	(1.19)
N. Obs.	6,916,855	6,916,855	6,916,855	6,916,855
Adj. R <sup>2</sup>	0.284	0.265	0.284	0.265
Cluster	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency
Firm-Agency FEs	Y	Y	Y	Y
Agency-Year FEs	Y	Y	Y	Y
Firm-Year FEs	Y	Y	Y	Y
Y Time Period	2002-2018	2002-2018	2002-2018	2002-2018
Sample	Res > 0, Fine Imposing	Res > 0, Fine Imposing	Res > 0, Fine Imposing	Res > 0, Fine Imposing
Y Sample Mean	0.0010693	0.0118394	0.0010693	0.0118394

Table 6. Procurement Contracts

The unit of observation is the firm-agency-year triplet. Panel A focuses on direct transitions, while Panel B focuses on indirect transitions. The dependent variable in Columns (1) and (4), *Contract Dummy*, is an indicator variable that takes the value of one if the agency in question signs a contract with the firm in a given year, and zero otherwise. The dependent variable in Columns (2) and (5),  $\ln(N \text{ Contracts} + 1)$  is the natural log of the number of contracts the agency in question signs with the firm in a given year, plus one. The dependent variable in Columns (3) and (6),  $\ln(\text{Contract Value} + 1)$ , is the total “Federal Action Obligation” across all contracts the agency in question signs with the firm in a given year, plus one. The sample includes all government contracts issued by 147 government agencies that could be matched to BoardEx. In Columns (1) through (3) the events analyzed are appointments of former regulators to top corporate positions, while in Columns (4) through (6) the events considered consists of departures of former regulators from the firm. *Event Year = t* denotes the time relative to appointment/departure event, in years. Panel A includes transitions to a top corporate position (i.e., the Event) that occur within three years since the individual leaves government. Panel B includes transitions to top corporate positions involving former regulators that join the firm more than three years since leaving government. The sample includes both public and private firms. T-stats based on standard errors clustered at the firm-agency level are reported in parentheses below the coefficients.

Panel A: Direct Transitions

Event type	Appointment of Former Regulator			Departure of Former Regulator		
	Contract Dummy	$\ln(N \text{ Contracts}+1)$	$\ln(\text{ContractValue}+1)$	Contract Dummy	$\ln(N \text{ Contracts}+1)$	$\ln(\text{ContractValue}+1)$
	(1)	(2)	(3)	(4)	(5)	(6)
Event Year = -2	0.001844 (0.40)	0.013947 (1.59)	0.007767 (0.14)	0.005848 (1.23)	0.014047 (1.43)	0.087514 (1.49)
Event Year = -1	0.004666 (1.07)	0.010050 (1.12)	0.044177 (0.84)	0.003250 (0.68)	0.003996 (0.39)	0.057302 (0.96)
Event Year = 0	0.005125 (1.16)	0.011240 (1.16)	0.065255 (1.18)	-0.000096 (-0.02)	-0.004397 (-0.42)	0.027008 (0.44)
Event Year = +1	0.009395** (2.18)	0.022167** (2.30)	0.112007** (2.01)	0.001569 (0.31)	-0.008695 (-0.78)	0.020490 (0.32)
Event Year = +2	0.001017 (0.22)	0.010560 (1.00)	0.020735 (0.35)	-0.000913 (-0.17)	-0.021522* (-1.73)	-0.036274 (-0.52)
Event Year >= +3	0.007755* (1.77)	0.002249 (0.20)	0.088692 (1.52)	-0.001422 (-0.28)	-0.025310* (-1.86)	-0.035427 (-0.52)
$\ln(\text{Restrictions} + 1) [t-1]$	0.000189* (1.96)	-0.000075 (-0.47)	0.001672 (1.57)	0.000191** (1.97)	-0.000071 (-0.45)	0.001691 (1.59)
$\ln(\text{Restrictions} + 1) [t]$	0.000082	-0.000284*	0.001406	0.000082	-0.000285*	0.001408

	(0.70)	(-1.78)	(1.11)	(0.70)	(-1.78)	(1.11)
ln(Restrictions + 1) [t+1]	-0.000041 (-0.35)	0.000351* (1.80)	-0.000783 (-0.60)	-0.000038 (-0.32)	0.000362* (1.85)	-0.000739 (-0.57)
N. Obs.	22,024,379	22,024,379	22,024,379	22,024,379	22,024,379	22,024,379
Adj. R <sup>2</sup>	0.527	0.753	0.600	0.527	0.753	0.600
Cluster	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency
Firm-Agency FEs	Y	Y	Y	Y	Y	Y
Agency-Year FEs	Y	Y	Y	Y	Y	Y
Firm-Year FEs	Y	Y	Y	Y	Y	Y
Y Time Period	2002-2018	2002-2018	2002-2018	2002-2018	2002-2018	2002-2018
Sample	Res > 0, Contracts	Res > 0, Contracts	Res > 0, Contracts	Res > 0, Contracts	Res > 0, Contracts	Res > 0, Contracts
Y Sample Mean	0.0051082	0.0080757	0.0580024	0.0051082	0.0080757	0.0580024

Panel B: Indirect Transitions

Event type	Appointment of Former Regulator			Departure of Former Regulator		
	Contract Dummy (1)	ln(N Contracts+1) (2)	ln(Contract Value+1) (3)	Contract Dummy (4)	ln(N Contracts+1) (5)	ln(Contract Value+1) (6)
Event Year = -2	0.004374* (1.82)	0.005248 (1.03)	0.042683 (1.46)	0.000182 (0.06)	0.001072 (0.19)	-0.000179 (-0.00)
Event Year = -1	0.002751 (1.12)	0.003525 (0.69)	0.024730 (0.84)	0.001151 (0.37)	0.005799 (0.92)	0.016295 (0.42)
Event Year = 0	0.002495 (0.95)	0.002003 (0.39)	0.034414 (1.06)	0.001379 (0.42)	0.004763 (0.71)	0.017779 (0.43)
Event Year = +1	0.003540 (1.39)	0.002003 (0.38)	0.051197 (1.63)	-0.000758 (-0.22)	0.000951 (0.13)	-0.016573 (-0.38)
Event Year = +2	0.000267 (0.10)	0.000333 (0.06)	0.007466 (0.23)	-0.002671 (-0.76)	-0.001318 (-0.16)	-0.021320 (-0.47)
Event Year >= +3	0.002228 (0.86)	-0.001869 (-0.33)	0.025464 (0.77)	-0.001507 (-0.41)	-0.007764 (-0.84)	-0.027634 (-0.58)
ln(Restrictions + 1) [t-1]	0.000190**	-0.000071	0.001682	0.000191**	-0.000071	0.001693

	(1.97)	(-0.45)	(1.58)	(1.97)	(-0.45)	(1.59)
ln(Restrictions + 1) [t]	0.000082	-0.000283*	0.001406	0.000083	-0.000282*	0.001416
	(0.70)	(-1.77)	(1.11)	(0.70)	(-1.76)	(1.12)
ln(Restrictions + 1) [t+1]	-0.000040	0.000357*	-0.000775	-0.000036	0.000364*	-0.000715
	(-0.34)	(1.83)	(-0.59)	(-0.31)	(1.86)	(-0.55)
N. Obs.	22,024,379	22,024,379	22,024,379	22,024,379	22,024,379	22,024,379
Adj. R <sup>2</sup>	0.527	0.753	0.600	0.527	0.753	0.600
Cluster	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency
Firm-Agency FEs	Y	Y	Y	Y	Y	Y
Agency-Year FEs	Y	Y	Y	Y	Y	Y
Firm-Year FEs	Y	Y	Y	Y	Y	Y
Y Time Period	2002-2018	2002-2018	2002-2018	2002-2018	2002-2018	2002-2018
	Res > 0,	Res > 0,	Res > 0,	Res > 0,	Res > 0,	Res > 0,
Sample	Contracts	Contracts	Contracts	Contracts	Contracts	Contracts
Y Sample Mean	0.0051082	0.0080757	0.0580024	0.0051082	0.0080757	0.0580024

Table 7. Contract Outcomes and The Revolving Door

The unit of observation is the contract level. Observations only include firm-agency-years in which contracts are signed. *Reneg Dummy* is an indicator that equals one if the price of the contract is renegotiated upwards at some point in the contract's life. *Reneg %* is the sum of all price renegotiations for the contract divided by the initial value of the contract. *Direct Transitions (t-1)* is an indicator for whether a former regulator was appointed (direct transition) the year before the contract in question was signed. *Contract Value* is the initial value of the contract in dollars. *N Offers* is the number of offers received by the agency for the contract during the bidding process. If *N Offers* is missing, it is set to one and the indicator *N Offers Missing* is set to one. Column (1) includes all contracts while column (2) only includes contracts that were renegotiated. The sample includes both public and private firms. T-stats based on standard errors clustered at the firm-agency level are reported in parentheses below the coefficients.

	Reneg Dummy (1)	Reneg % (2)
Direct Transitions (t-1)	-0.005215* (-1.88)	-0.198784 (-0.96)
ln(Restrictions + 1)	-0.006113*** (-3.77)	-0.240046* (-1.93)
ln(Contract Value + 1)	0.002969*** (3.27)	-0.163551*** (-6.63)
ln(N Offers + 1)	0.003530*** (2.98)	0.107036*** (4.34)
N Offers Missing	0.005892*** (2.94)	0.124728 (0.79)
Observations	13,436,263	89,984
Adj. R <sup>2</sup>	0.193	0.209
Cluster	Firm-Agency	Firm-Agency
Award Type FE	Y	Y
Firm-Agency FE	Y	Y
Agency-Year FE	Y	Y
Firm-Year FE	Y	Y
Y Time Period	2002-2018	2002-2018
Sample	Res > 0	Res > 0, Reneg = 1

Table 8. Election Years

The unit of observation is the firm-agency-year triplet. The tables focus on direct transitions. In column (1) *Fines Dummy* is an indicator taking the value of one if a fine imposed on firm *i* by agency *a* during year *t*, and zero otherwise. In columns (2)  $\ln(\text{Fines Amount}+1)$  is the natural log of the amount of fines imposed on firm *i* by agency *a* during year *t*, plus one. In column (3), *Contract Dummy* is an indicator variable that takes the value of one if the agency in question signs a contract with the firm in a given year, and zero otherwise. In column (4),  $\ln(N \text{ Contracts} + 1)$  is the natural log of the number of contracts the agency in question signs with the firm in a given year, plus one. Finally, in column (5),  $\ln(\text{Contract Value} + 1)$  is the total “Federal Action Obligation” across all contracts the agency in question signs with the firm in a given year, plus one. *Event Year = t* denotes the time relative to appointment/departure event, in years. *Election Year = t* denotes the time relative to a U.S. Presidential election. The sample includes both public and private firms. T-stats based on standard errors clustered at the firm-agency level are reported in parentheses below the coefficients.

	Fines Dummy (1)	$\ln(\text{Fines Amount}+1)$ (2)	Contract Dummy (3)	$\ln(N$ Contracts+1) (4)	$\ln(\text{Contract}$ Value+1) (5)
Event Year = -2	0.003924 (0.67)	0.056178 (0.63)	0.002621 (0.51)	0.012323 (1.38)	0.003601 (0.06)
Event Year = -2 x Election Year = -1	-0.004684 (-0.44)	-0.125339 (-0.82)	-0.003049 (-0.32)	0.005220 (0.25)	0.012464 (0.10)
Event Year = -1	0.003783 (0.70)	0.044279 (0.55)	-0.001434 (-0.31)	0.005137 (0.55)	-0.017918 (-0.31)
Event Year = -1 x Election Year = 0	-0.013229 (-1.54)	-0.225677* (-1.89)	0.024002** (2.52)	0.019374 (0.99)	0.243923** (2.19)
Event Year = 0	0.022607*** (3.22)	0.345918*** (3.20)	0.002882 (0.62)	0.010084 (1.06)	0.031543 (0.54)
Event Year = 0 x Election Year = +1	-0.019239 (-1.56)	-0.344061* (-1.93)	0.009171 (0.93)	0.005613 (0.28)	0.137938 (1.12)
Event Year = +1	0.002099 (0.39)	0.028531 (0.35)	0.009070* (1.88)	0.020569** (2.08)	0.106374* (1.75)
Event Year = +1 x Election Year = +2	0.014555 (1.09)	0.164748 (0.87)	0.002016 (0.22)	0.010294 (0.57)	0.032903 (0.28)
Event Year = +2	0.003840 (0.71)	0.056743 (0.68)	-0.001135 (-0.22)	0.009408 (0.90)	-0.000463 (-0.01)
Event Year = +2 x Election Year = +3	0.005714	0.077597	0.012961	0.013011	0.131338

	(0.43)	(0.38)	(1.25)	(0.57)	(1.07)
Event Year $\geq +3$	0.001177	0.018145	0.017161**	0.032598**	0.210538**
	(0.23)	(0.23)	(2.26)	(2.00)	(2.18)
Event Year $\geq +3$ x Elect. Year $\geq +4$	0.005867	0.067526	-0.010367	-0.032712**	-0.134040
	(1.55)	(1.18)	(-1.52)	(-2.49)	(-1.57)
ln(Restrictions + 1): t-1	0.000072	0.001520	0.000189*	-0.000075	0.001670
	(0.69)	(1.17)	(1.96)	(-0.47)	(1.57)
ln(Restrictions + 1): t	-0.000041	-0.000168	0.000082	-0.000284*	0.001404
	(-0.25)	(-0.09)	(0.69)	(-1.78)	(1.11)
ln(Restrictions + 1): t+1	0.000138	0.002144	-0.000040	0.000353*	-0.000776
	(0.89)	(1.13)	(-0.35)	(1.81)	(-0.59)
Observations	6,916,855	6,916,855	22,024,379	22,024,379	22,024,379
Adj. R <sup>2</sup>	0.284	0.265	0.527	0.753	0.600
Cluster	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency
Firm-Agency FE	Y	Y	Y	Y	Y
Agency-Year FE	Y	Y	Y	Y	Y
Firm-Year FE	Y	Y	Y	Y	Y
Y Time Period	2002-2018	2002-2018	2002-2018	2002-2018	2002-2018
Sample	Res > 0, Fine Imposing	Res > 0, Fine Imposing	Res > 0, Contracts	Res > 0, Contracts	Res > 0, Contracts
Y Sample Mean	0.0010693	0.0118394	0.0051082	0.0080757	0.0580024



Figure 1. The Revolving Door across Industries and over Time

The figure depicts the evolution of the appointment of former regulators across broad industries during 2002-2018. Appointment of former regulators is measured at the company level and is based on individuals who cover top corporate positions and have transitioned to the firm within three years since leaving an agency.

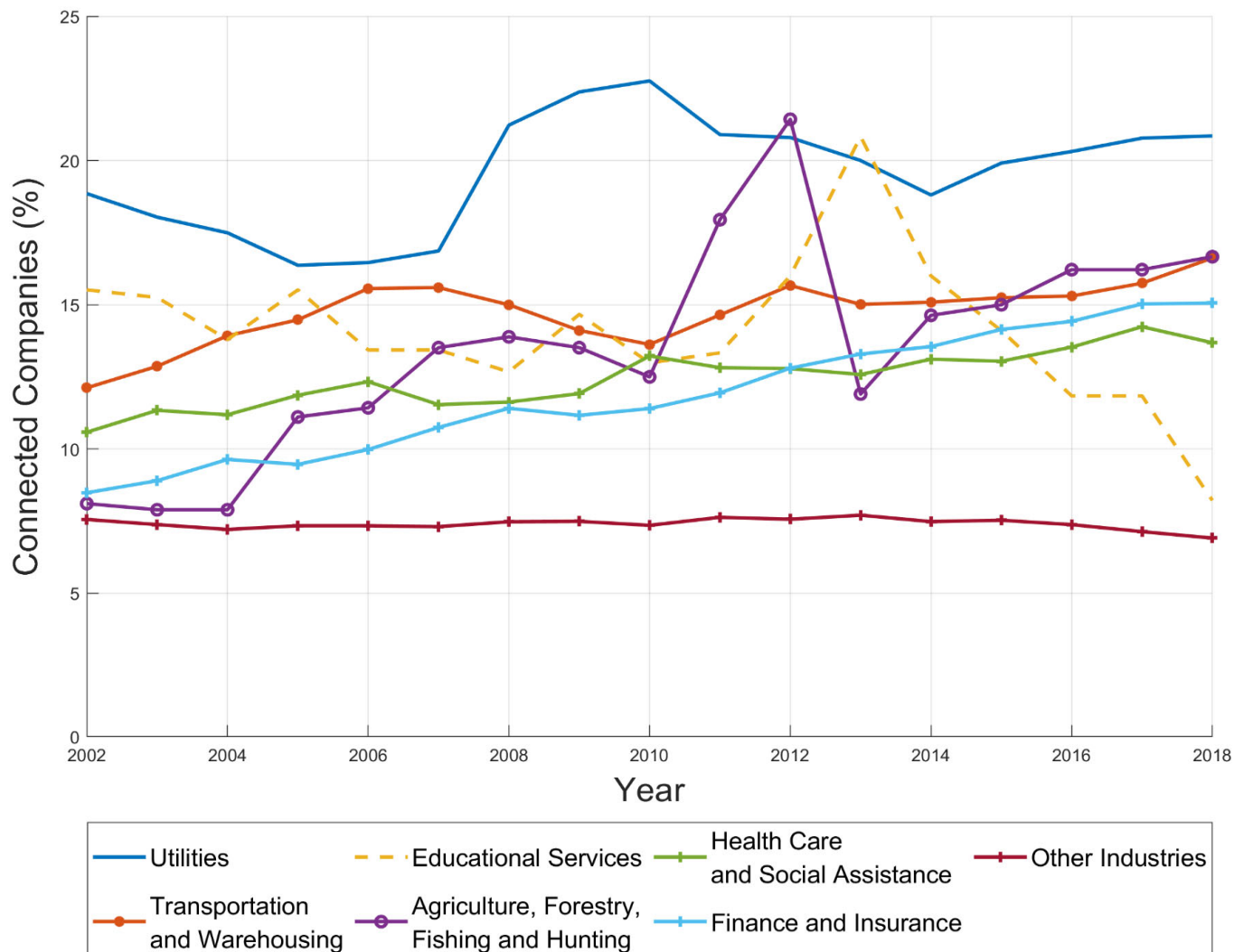


Figure 2. The Revolving Door across States

The figure depicts averages of the appointment of former regulators across U.S. states during 2002-2018. Appointment of former regulators is measured at the company level and is based on individuals who cover top corporate positions and have transitioned to the firm within three years since leaving an agency. A darker color indicates a higher percentage of firms headquartered in the State in question with at least one such individual.

